The Functional Analysis of Psychological Terms: In Defense of a Research Program

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In 1945, B. F. Skinner outlined a proposal that psychological or mentalistic terms found in natural language might be analyzed empirically in terms of the variables, conditions, and contingencies of which they may be observed to be a function. Such an analysis would enable discriminations to be made between different classes of variables that enter into the control of the term. In this way, the analysis would clarify what is traditionally called the "meanings" of such terms as they occur as properties of verbal behavior. Despite his expressed confidence in the success of such a program, Skinner largely abandoned the functional analysis of psychological terms in favor of the development of a promising new field; the experimental analysis of behavior. The present paper argues that the original program is of great importance as well, and for the following reasons: (a) to make full, immediate, and (most importantly) effective contact with the range of issues and terms of central importance to the traditionally and culturally important concepts of "mind" and "mental life" (and thereby demonstrating the relevance of radical behaviorism to the full range of human and verbal behavior); and (b) to extend the methodology of the functional analysis of verbal behavior more generally. Such a research program would demonstrate, through an empirically-based scientific analysis, that the philosophical problems concerning "mental life" may be productively analyzed as problems of verbal behavior. Issues of methodology are discussed, and possible methodological strategies are proposed regarding the confirmation of behavior analytic interpretations of mentalistic terms.

In 1945, a paper by B. F. Skinner which was to become a cornerstone in the development of behavior analytic science appeared in the *Psychological Review*. The paper was based upon Skinner's contribution to the Harvard Symposium on Operationism, and was entitled, "The Operational Analysis of Psychological Terms" (reprinted in Skinner, 1972; page numbers cited in the current paper are taken from the original source).

Skinner's 1945 paper was an unusual and important contribution in several ways. It was the first published source in which it was clear that Skinner's interpretation of operationism in psychology was radically different from what was to be the standard, Boring-Stevens interpretation (e.g., Moore, 1975). Second, the paper provided Skinner's first description of a functional analysis of verbal behavior; a

prelude to his 1957 book, *Verbal Behavior*. Third, and more specifically on the theme of verbal behavior, the paper provided a relatively detailed description of Skinner's interpretation of verbal behavior under the control of private events. Fourth, it is significant in the context of the above three themes that the paper appears to be the first time Skinner employed the term "radical behaviorism" in describing his systematic scientific position (to be distinguished from what he termed, "methodological behaviorism"; e.g., Day, 1980/1992, 1983/1992).

In addition to such well-known themes, however, Skinner's 1945 paper also described a program of research; one which had been of great interest to Skinner during his predoctoral days at Harvard. The proposal was described by the title of the paper; that is, the operational, or as we would now say, functional, analysis of psychological terms (e.g., Moore, 1975). More specifically, it was Skinner's proposal that subjective, psychological, or mentalistic terms found in natural language could be

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analyzed in terms of the variables, conditions, or contingencies of which they may be observed to be a function. One of the effects of such an analysis would be to clarify what is traditionally called the "usages" or "meanings" of a term in the sense of how the term occurs as a verbal response class in contact with verbal and nonverbal contingencies (e.g., Skinner, 1945, 1957; possible implications of such analyses will be discussed in more detail below). The purpose of this paper is to defend the research program described by Skinner (1945), and to suggest certain methodological strategies which might be pursued in the development of such a program.

Skinner's first systematic attempt at such an analysis may be seen in his doctoral dissertation (Skinner, 1931), in which the term "reflex" was examined in historical and contemporary context with an interest in an interpretation of the types of variables controlling the occurrence of the term. On the basis of his analysis, Skinner (1931) made the case that the necessary conditions for the occurrence of the term "reflex" was an observed correlation between a stimulus and a response. Thus, the "physiological" terms which frequently accompanied the term "reflex" during this period (e.g., an assumed or "inferred," as opposed to observed, "synapse"), could be regarded as superfluous and possibly misleading verbal behavior, when considered in the context of scientific discourse (see also Skinner, 1957).

Although Skinner's early treatment of "reflex" remains his most detailed interpretive example of a functional analysis, it is worth noting that during this period he had also proposed a more extensive program of research. It is by now a well-known story (recounted in Skinner, 1945) that when Skinner had been looking toward preparation for his doctoral examination at Harvard, he had approached a member of the psychology department faculty with a proposal: "Unmindful or ignorant of the ethics of the academy, I suggested. . .that if I could be excused from anything but the most perfunctory examination, the time which I would otherwise

spend in preparation would be devoted to an operational analysis of half-a-dozen key terms from subjective psychology" (Skinner, 1945, p. 291). Skinner's proposal was not met with enthusiasm, but the following statement in Skinner's (1945) account of the episode is significant:

> The point I want to make is that at that time – 1930 – I could regard an operational [functional] analysis of subjective terms as a *mere exercise in scientific method.* . . . It never occurred to me that the analysis could take any but a single course or have any relation to my own prejudices. The result seemed as predetermined as that of a mathematical calculation.
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> . . . I am of this opinion still. (Skinner, 1945, pp. 291-292, emphasis in original)

Skinner's confidence in the success of such a program may seem surprising since (a) the "exercise" would appear to be an extraordinarily complex undertaking even by contemporary standards, and (b) the "scientific method" to be employed in such an exercise is not apparent; for example, it does not appear as though Skinner is referring to an obvious variant on traditional operant experimental methodology (cf. Brinker & Jaynes, 1988).

THE FATE OF SKINNER'S PROPOSAL

Given Skinner's enduring confidence in the functional analysis of mentalistic or psychological terms as a program of empirical research, the question arises as to the fate of Skinner's program. In one sense, as we will see, Skinner provided examples of such analyses throughout many of his writings. Such examples may be found in Skinner's interpretive writings where ordinary-language, mentalistic expressions are "translated" into the technical vocabulary of contingencies of reinforcement (e.g., Skinner, 1953, 1957, 1974). Examples and issues arising from such interpretive practices will be examined briefly in the next section.

For the present, however, it is clear that Skinner had never pursued the functional analysis of psychological terms in the sense of a systematic program of empirical research; this despite the fact that he appeared to have complete confidence in the success of such a program. In his 1945

paper, Skinner neither developed nor promoted the development of such a research program. Essentially, this was because he felt that the program was not worth doing, given that a more productive alternative program of scientific research was available. Skinner (1945) summarized the argument in the following way:

> most of the early behaviorists, as well as those of us just coming along who claimed some systematic continuity, had begun to see that psychology actually did not require the redefinition of subjective concepts. The reinterpretation of an established set of explanatory fictions was not the way to secure the tools then needed for a scientific description of behavior. Historical prestige was beside the point. . . . They might as well have spent their time in showing what an eighteenth-century chemist was talking about when he said that the Metallic Substances consisted of vitrifiable earth united with phlogiston. There was no doubt that such a statement could be analyzed operationally or translated into modern terms, or that subjective terms could be operationally defined [in the sense of a functional analysis of verbal behavior]. But such matters were of historical interest only. What was wanted was a fresh set of concepts derived from a direct analysis of the newly emphasized data. (p. 292)

It was Skinner's position in 1945 that the functional analysis of mentalistic terms was an unnecessary and superfluous scientific enterprise, in that the principal scientific need was not to analyze and thus to "understand" traditional mentalistic expressions found in ordinary language in terms of the technical vocabulary of contemporary behavioral science. The principal task of a science of behavior (as it has been for chemistry, to follow Skinner's example in the quote above) was to develop new terms, new concepts, and new methods based on the development of an effective, efficient, and comprehensive science of behavior.

It could thus be said that in Skinner's 1945 paper, the program first proposed was then abandoned as a potential distraction from the primary task of the newly-developing "experimental analysis of behavior." However, it has also been noted that Skinner made extensive use of the *interpretation* of mentalistic terms and expressions in his writings, and it is to the practice of such "translations" that we now turn.

ON SKINNER'S "TRANSLATIONS"

Skinner's extensive writings include many examples of the interpretation of ordinary-language mentalistic terms or expressions in terms of the technical vocabulary of contingencies of reinforcement (e.g., Skinner, 1953, 1957, 1964, 1969). A particulary rich source of such interpretive statements or "translations" from one vocabulary into the other may be found in Skinner's (1974) *About Behaviorism* (see also Day, 1976b/1992). For example, in this book the chapter entitled "Operant Behavior" is almost entirely an interpretive exercise which focuses upon ordinary-language expressions involving "purpose," "purposive," and related terms. The following quotes are examples of such "translations" taken from various parts of the book:

> Frustration is rather a different condition, which includes a tendency, often characteristic of a failure to be reinforced, to attack the system. . . . The expression "frustrated expectations" refers specifically to a condition produced by the termination of accustomed reinforcement. (Skinner, 1974, p. 58)

> Most reinforcements occur intermittently, and the schedules on which they are programmed generate conditions which are described with a wide range of terms. The so-called ratio schedules supply many good examples. When the ratio of responses to reinforcements is favorable, the behavior is commonly attributed to (1) diligence, industry, or ambition, (2) determination, stubbornness, staying power, or perseverance (continuing to respond over long periods of time without results), (3) excitement or enthusiasm, or (4) dedication or compulsion. (Skinner, 1974, p. 59)

> The other Freudian dynamisms or defense mechanisms may be treated in the same way. They are not psychic processes taking place in the depths of the mind, conscious or unconscious; they are the effects of contingencies of reinforcement, almost always involving punishment. At best we may say that they are ways in which a persons defends himself against punishment by acquiring behavior effective in the world in which he lives (as ego), reinforced in part because of suseptibilities to reinforcement which are part of his genetic endowment (as id), and not punished by other persons or by himself (as superego). . . . It has been said that "inhibiting forces which oppose the discharge of tension are the immediate subject of psychology," and if this is true, it is only because inhibiting forces and the discharge of tension are figures of speech referring to punishment and reinforcement, respectively. (Skinner, 1974, p. 157)

It is worth noting that such "translations" have been a source of controversy and a frequent target of Skinner's critics. For example, in Koch's (1976) review of Skinner's (1974) *About Behaviorism*, Skinner is frequently taken to task for the reduction of complex mentalistic expressions to the language of physics, or to "physical thing language." Malcolm (1964) has also criticized Skinner (e.g., 1953) for what appears to be the physicalistic reduction involved in such translations. Willard Day has described how such criticisms of Skinner's translations involve misguided assumptions regarding Skinner's radical behaviorism (e.g., see Day's reply to Koch, 1976; Day, 1976c/1992, and to Malcolm, 1964; Day 1977/1992).

In making an interpretive "translation" from natural language to a technical scientific vocabulary, Skinner is not "reducing" the terms or concepts to some sort of physical foundation. This is because the technical vocabulary of behavior analytic science is itself not reducible to the language of physics (i.e., movement through space over time; for discussions see, e.g., Catania, 1992; Day, 1976c/1992, 1977/1992; Leigland, 1993). The technical terms of behavior analysis (such as "reinforcement," "discriminative stimulus function," etc.) may be viewed as abstractions (or abstract tacts; Skinner, 1957) in which names are applied to *observed relations* between environment and behavior. No "physical" foundation may be found in such cases, since questions regarding the technical terms themselves become questions regarding observed relations between environment and behavior (rather than simply "physical" movements through space over time).

What was Skinner doing, then, when such translations were made? Rather than physicalistic reduction, it could be said that the translations involve *pragmatic reformulation*. That is, it is not the case that the mentalistic language is "not meaningful," or that it is not "what is really happening" in some ontological sense (although in Skinner's shorthand or summary presentations, some ontology may appear to be

implied by the use of such phases as, "They are not. . .," in the third example quoted above). It is also not the case that the technical scientific vocabulary is, in some sense, the "underlying truth" to which the natural language may be reduced, or is the foundation upon which the "meaning" of the natural language statement rests. From the perspective of radical behaviorism, both types of statments are "meaningful," but may be said to serve different functions depending upon context. The issues may be illustrated by looking at another quote from Skinner, originally chosen for criticism by Malcolm (1964), and included in the response by Day (1977/1992):

> *It is of no advantage to say* that [occupational] therapy helps the patient by giving him a "sense of achievement" or improves his "morale," builds up his "interest," or removes or prevents "discouragement." Such terms as these merely add to the growing population of explanatory fictions. One who readily engages in a given activity is not showing an interest, he is showing the effect of reinforcement. We do not give a man a sense of achievement, we reinforce a particular action. To become discouraged is simply to fail to respond because reinforcement is not forthcoming. (Skinner, 1953, p. 72, emphasis added)

The thrust of Skinner's argument may be seen in the first seven words of the passage. The primary function of Skinner's translations is to provide interpretive examples of how traditional issues stated in traditional terms may be brought into the productive and effective domain of a natural science of behavior. Ordinary language functions well enough in most cases of ordinary discourse, but when effective action is required, successful working is needed, human problems must be solved, behavior is to be changed, a more precise way of describing and communicating is required (Skinner, 1957). A look at the specialized technical vocabulary of chemistry, for example, clearly shows that the argument applies not only to technology and applied science, but to basic science as well (see also Sidman, 1960, 1989; Skinner, 1953, 1957).

In Day's (1976c/1992) reply to Koch's (1976) review, it is noted (somewhat sardonically and with great irony) that

despite Koch's rather bitter and unrelenting criticism of Skinner's interpretations, research program, and views of science, Koch himself had expressed interest in the kind of program for the analysis of verbal behavior which had been described by Skinner (1945). At the Symposium on Behaviorism and Phenomenology held at Rice University in 1964, Koch's (1964) contribution was a lengthy attack upon "behaviorism" in psychology (although in this case it was a detailed criticism of mediational neobehaviorism, rather than Skinner's radical behaviorism; e.g., Day, 1969b/1992). In concluding the section (taken from his presentation), Koch (1964) described what he viewed as interesting trends in psychology which might be productive alternatives to behaviorism. One of these recommendations appears in a section entitled, "New Conceptions of Definition and Meaning," as follows: "What I think necessary, at least as a preliminary, is relatively simple-minded *empirical* analysis of the conditions of communication and of actual definitional practice in the natural languages" (Koch, 1964, p. 25, emphasis in original). This was, of course (and apparently unknown to Koch, 1964), a good summary of Skinner's (1945) proposal for the functional analysis of (natural language) mentalistic terms.

To summarize, in Skinner's 1945 paper on "operationism," a program of research was described for the empirical analysis of ordinary-language "mentalistic," "subjective," or "psychological" terms with respect to the contingencies which may be observed to control their occurrence as properties of verbal behavior. Skinner (1945) expressed a remarkable degree of confidence in the success of such a research program, but despite the fact that during his career he contributed many illustrative "translations" as possible interpretative examples, he essentially abandoned the program in the latter part of the 1945 paper. Skinner abandoned the program because he saw no need to analyze and clarify terms which had been part of an ineffective tradition, when new and more

effective scientific terms and analyses were needed to continue the progress of a promising new field; the experimental analysis of behavior. Although there is no question about the need for continued research in all areas of behavior analytic science (as Skinner advocated in the 1945 paper), a case may also be made for the importance of adding the program that had captured Skinner's early interests; the functional analysis of psychological terms.

THE NEED FOR THE PROGRAM

What would such a research program accomplish? Would it truly be, as Skinner (1945) indicated, of historical interest only? There are two issues upon which the program might be defended. First, the functional analysis of psychological terms would bring behavior analytic science into full, immediate, and (most importantly) effective contact with the full range of issues, problems, and concerns of the larger arena of philosophers, cognitivists, and lay culture. To state the matter bluntly, the concept of "mind" and the issues of the "mental" are of central concern to Western culture; this despite persistent and intelligent critiques by not only Skinner (e.g., 1971), but also by philosophers such as Gilbert Ryle (1949) and Richard Rorty (1979). Yet reasoned arguments and practical concerns have done little to promote the broader consideration of alternative perspectives regarding human functioning. A different strategy would be to critically examine the concepts of mind, mental life, the subjective and psychological, not as "real" entities or "existent" properties or essences but rather as verbal behavior and the products of verbal processes.

It is clear that Skinner saw the value of such an exercise. Consider the following series of quotations:

We may quarrel with any analysis which appeals to. . .an inner determiner of action, but the facts which have been represented with such devices cannot be ignored. (Skinner, 1953, p. 284)

We are interested in finding terms, not to take traditional places, but to deal with a traditional subject matter. (Skinner, 1957, p. 115)

No entity or process which has had any useful or explanatory force is to be rejected on the ground that it is subjective or mental. The data which have made it important must, however, be studied and formulated in effective ways. The assignment is well within the scope of an experimental analysis of behavior, which thus offers a promising alternative to a commitment to pure description on the one hand and an appeal to mentalistic theories on the other. (Skinner, 1964, p. 96)

For Skinner, the interests and problems of *traditional* mentalistic psychology would eventually be addressed and reformuled in *effective* ways by the methods and programmatic advances of behavior analytic science.

As we have seen, the interest for Skinner in such connections was also apparent in his use of his interpretative "translations" from traditional to scientific terms. As a third line of evidence, Skinner also made frequent use in his later years of etymology as a means of illustrating the complex behavioral histories involved in the evolution of common mentalistic terms (a general review is provided in Skinner's, "The Origins of Cognitive Thought," 1989).

The function of the etymological exercise is to make conspicuous the evolutionary, dynamic, and contextual nature of the terms which are frequently taken "at face value" in contemporary discourse on mental life. In such discourse, complex issues of language are typically finessed through the adoption of a commonsense realism regarding terms from folk psychology. This is often seen in the cognitive literature, where terms like "thinking," "perception," "attention," "learning," and the like are treated as if they refer to singular, unitary, real processes of brain function (e.g., Gardner, 1985), rather than viewed as conventions of "usage" (i.e., verbal interactions) with complex histories and complex relations to contextual variables. In other words, the etymological exercise may serve to promote a *behavioral* view of language when mentalistic talk is of interest.

In carrying out a functional analysis of psychological terms, the goal would not be, of course, to bring traditional terms into the technical discourse of behavior analysis. The goal would rather be to bring behavior analytic science to the traditional terms and problems, and thereby (1) demonstrate the behavioral character of such terms and problems, and (2) clarify some of the variables and interactions involved with the occurrence of such terms and problems (see also Day, 1969a/1992).

Further, to engage such a research program would raise a number of methodological issues which could broaden the scope of the analysis of verbal behavior more generally. While the field of verbal behavior has seen a great deal of progress, particularly in recent years, issues concerning the kinds of research methods appropriate to the functional analysis of verbal behavior remain a matter of discussion. For example, while a number of variations on experimental methods have appeared (e.g., Drash & Tudor, 1991; Leigland, 1991a, 1991b; Salzinger, Portnoy, Zlotogura, & Keisner, 1963; Sundberg, San Juan, Dawdy, & Arguelles, 1990), the general applicability of traditional experimental approaches has been questioned in the larger domain of verbal phenomena (e.g., Brinker & Jaynes, 1988; Czuberoff, 1993; see also Moore, 1991).

The point is that empirical methods may be needed in addition to traditional experimental approaches as the analysis of verbal behavior expands in scope and domain. Skinner's (1945) confidence in the successful pursuit of the functional analysis of mentalistic terms may be viewed as a challange for methodological development. As we will see, Skinner also recognized the technical difficulties of carrying out such a program, and in fact, recent research in equivalence phenomena (e.g., Sidman, 1994) may indicate that there are more methodological difficulties than Skinner had suspected (cf. Hayes, 1994; Hayes & Wilson, 1993). Nevertheless, there are reasons to maintain that substantial progress can be made in the functional analysis of psychological terms. These issues will be taken up again in the latter part of this paper.

An Analogy: Eighteenth-Century Chemistry

In making the case for Skinner's (1945) research program, it may be helpful to con-

sider an analogy. The analogy is rather fanciful, but it has the advantage of originating with Skinner's 1945 paper. Recall from a passage quoted above that Skinner's view of the pursuit of the functional analysis of mentalistic terms would be analogous to ". . .[spending] time in showing what an eighteenth-century chemist was talking about when he said that the Metallic Substances consisted of vitrifiable earth united with phlogiston. There was no doubt that such a statement could be analyzed operationally or translated into modern terms. . . . But such matters were of historical interest only" (Skinner, 1945, p. 292). It may be true that the exercise, when carried out in the field of chemistry, might be of historical interest only. There might be benefits if carried out in the field of human behavior, however.

Imagine that a contemporary chemist has the opportunity to be transported in time back to the eighteenth century, with all of the necessary equipment, for the purposes of introducing some of the people of that age to the chemistry of today. How should the introduction be made upon arrival? One alternative would be for the chemist to simply pull out the equipment and demonstrate some state-of-the-art chemical science and technology, to the effect of dazzling the prescientific chemists into curiousity and acceptance. One can easily imagine, however, that a possible consequence of a demonstration of such witchcraft would be for the chemist to be burned at the local stake.

As an alternative, some ground might be gained by making contact with the chemistry of the day. For example, in learning of eighteenth century chemistry from the locals, the chemist is informed that the metallic substances consist of vitrifiable earth united with phlogiston. The chemist need not contend that such a statement is not true, but rather that there is "another way to say the same thing" (or essentially the same thing). Unfortunately, the locals are told, this new way of talking about such things is complex, and requires much learning. If the learning is undertaken by the locals, and they are subsequently con-

vinced that there are indeed two ways to talk about such a phenonenon, the question becomes, "Why would one choose a more complex way of talking about the Metallic Substances when a simpler one will do?" It is here that the chemist introduces the advantages of the contemporary scientific approach: *"Because look at what else I can do when talking and working this complex way compared to the simpler way,"* and the demonstrations begin.

The analogy may be drawn by comparing this situation to one in which a behavior analyst learns from her cognitivist colleague in psychology or philosophy that "intentionality is the mark of the mental." With an adequate functional analysis of the term, "mental," and also of the technical problem of "intentionality" in philosophy (e.g., Bechtel, 1988), the original statement might be re-cast into the technical vocabulary of behavior analytic science. This would be a massive undertaking, to be sure, but if an equivalence between the two such statements could be *demonstrated empirically*, the effectiveness of the technical formulation would have been demonstrated in a particularly compelling fashion, having described a *traditional* issue in a vocabulary which brings with it the power of an *effective scientific analysis*.

It is worth emphasizing that, with an adequate functional analysis, the two vocabularies (the lay and the technical) could be considered "equivalent" in the sense that they are under the control of the same phenomena, or are "talking about the same thing," or are "making the same (general) kinds of distinctions." The two vocabularies would not be "equivalent" in the sense of complete interchangability or substitutivity under all conditions, since the two vocabularies serve different functions; one serves the expedient function of ordinary discourse, while the other serves the scientific/technical function of enabling practical action with respect to verbal and nonverbal behavior. It is important to note that neither vocabulary is viewed as the "foundation" for the other in any ontological or epistemological sense; rather, they are simply "different ways of

talking about the same things," to state the matter informally.

The relation between the lay and technical vocabularies may be illustrated with an example. One of the common criticisms brought by cognitivist philosophers against Skinner's work involves the use of intentional idioms (e.g., Dennett, 1978; Flanagan, 1991). Essentially, the alleged problem is that Skinner's science forbids the use of intentional idioms as part of the vocabulary of the science, yet an understanding of the former is required and necessary if the latter is to be understood. Flanagan (1991) has summarized the issue in the following way:

> The true behavioral laws Skinner comes up with in situations such as [the operant experimental laboratory] make sense precisely because there are true mentalistic laws which underlie them. That the animal pecks or paws at rate x in the presence of stimulus s on schedule r *makes sense* because we know that any organism at 80 percent of normal weight is hungry and desires food. (Flanagan, 1991, pp. 96-97; emphasis added)

In other words, it is maintained that intentional terms such as "desires," "believes," and "expects," are unavoidable and should be conceded as part of the necessary conceptual apparatus to be employed in psychological explanation, since such terms form the basis for the understanding of any alternative formulation.

The point is that whether or not intentional idioms may, under certain conditions, be useful in making predictions of behavior or informal summary-descriptions of the results of particular classes of environment-behavior interactions, the fact remains that for effective action and the successful arrangement or control of events, a more precise vocabulary is characteristically necessary (e.g., Skinner, 1957). Intentional idioms may be seen to occur in many contexts, such as when we look at the sky and say that, "It's thinking about raining," or that, "The weather can't make up it's mind what to do today," but such cognitive meteorology is not likely to compete with the contemporary scientific varieties when it comes to analysis and prediction.

Similarly, while Flanagan would feel most comfortable in describing the events in an operant chamber with the use of the standard intentional idioms (e.g., "The pigeon's stopped pecking because he's discouraged about not getting any food," or "...because he knows he won't get food any more"), it would be most unlikely if Flanagan could *arrange or produce* any of the complex behavioral phenomena of the undergraduate operant laboratory without the benefit of the technical vocabulary and the special history of training associated with it. As in other scientific fields, the issue is not that the ordinary language discourse is somehow made meaningless or invalid by the development of a technical scientific vocabulary, it is rather that the new ways of speaking carry with them the practical power of a precise and detailed scientific analysis (see also Baum & Heath, 1992).

The case for Skinner's 1945 program might be summarized in the following way: in a culture which values the terms, issues, and problems formulated in a traditional mentalistic vocabulary, the functional analysis of mentalistic terms may be a conspicuous inroad for the advancement of an alternative scientific formulation. The potential effects of such an analytic program would be to (a) demonstrate the behavioral character of the traditional problems and terms, (b) provide an alternative and empirically-based formulation of the traditional problem which brings with it an effective analysis of psychological/behavioral phenomena more generally, and (c) provide an opportunity for methodological development in the field of behavior analysis in general and the analysis of verbal behavior in particular.

Possible Candidates for Analysis

Of the myriad types of "mentalistic," "subjective," or "psychological" terms which might make interesting or productive candidates for a functional analysis, three types or categories of terms will be discussed briefly. These are (a) terms relevant to the practice of science; (b) terms of importance to traditional issues of "mind," and in particular, "mind-body" (or "mental-physical") relations; and (c) terms of

importance to the traditional fields of metaphysics and ontology.

The practice of science might benefit from a functional analysis of some key terms (e.g., Skinner, 1945, 1957). For example, the issue of "theory" has been a recent topic of discussion in behavior analysis (e.g., Staddon, 1993a, 1993b, and associated commentaries). Of the many terms of central importance to the history of psychological theory, the term "inference" is one which occurs frequently in such discussions and which might benefit from clarification (particulary as it frequently contrasts with "observation," and perhaps in its relation to "abstraction"). Also, an analysis of the terms "objective" and "subjective" might serve to clarify some issues of central importance to science more generally (see the recent review of the distinction between the two terms from the perspective of radical behaviorism by Moore, 1995).

As noted above, Skinner's 1945 program emphasized the analysis of "psychological" terms, and certainly the vocabulary of mental life would supply many fascinating candidates for analysis (e.g., Block, 1980, 1981; Flanagan, 1991). For example, the technical issue of "intentionality" (to be distinguished from "intention") is of great importance to discussions of "mind" and the "mental" (e.g., Bechtel, 1988), and while some interpretative work on intentionality has been proposed (e.g., Day, 1976b/1992), there are some properties of the issue which might make it a good candidate for a functional analysis (e.g., Leigland, 1992). Many other terms and issues are of interest as well (for a summary and critique of the concept of mind, see Rorty, 1979).

A particularly challenging but useful set of terms to be considered for analysis would be those of central importance to issues of ontology. It could be said that many of the most important philosophical issues revolve around these terms, as have many scientific debates, confusions, and problems. Three conspicuous examples would be "is," "real," and "exists."

An example may be put into verbal context by returning to the problem of inten-

tionality. It is said, for example, that a distinguishing characteristic of the mental is that, unlike physical objects, mental events are *about* something outside of themselves (or that they "point to," or "intend" things or events outside of themselves). One does not simply "believe" or "think," but rather must "believe X" or "think about Y" (e.g., Bechtel, 1988). Further, one of the problematic implications of intentionality is the observation that intentional objects need not exist, as when one is thinking about a unicorn (to take a standard example). However, it is clear that such issues and problems depend critically upon the "meanings" or "usages" of such terms as "exist." An empirically-based, functional analysis of such a term might clarify the histories, variables, and conditions which are important for the functions of such verbal problems.

It is likely that equivalence phemonena would be important to the analysis of such terms (e.g., Hayes & Hayes, 1992; Sidman, 1994). This may be most conspicuously the case in the example of "is." The following interpretive comments by Catania (1992) are useful by way of illustration:

> the verb *is* serves many functions. Sometimes it specifies that the verbal response it accompanies is a tact ("This is a book") [see Skinner, 1957], sometimes it prescribes equivalences between verbal responses ("A human is a featherless biped"), and sometimes it specifies temporal properties ("It is cold now"). The particular function of is often depends on other verbal responses or, in other words, on context. Not only does it function as a conditional stimulus with respect to the effects of other verbal behavior, but its function may in turn be conditional on other verbal behavior. (Catania, 1992, p. 246)

Such examples are, of course, extraordinarily complex, and present a host of methodological challenges. While Skinner (1945) had not addressed specific questions of methodological development, we may nevertheless consider some of the issues involved and possible methodological strategies.

ISSUES OF METHODOLOGY

In his discussion of the abstract tact, Skinner (1957) made the following remarks

concerning functional-analytic methodology:

> The referents of abstractions – the properties of stimuli which control abstract tacts – can be discovered only by certain methods of empirical investigation. What do *pyramidality, poetry, chair, red,* or *foxy* really "mean"? If we try to answer this by discovering what they "mean to us," we are behaving empirically, although under a certain handicap. It is easier to discover what they "mean" to someone else. There are many technical problems to be solved before this can be done on a satisfactory scale, but the basic formula is simple: manipulate stimuli and, through the presence or absence of the response, identify the effective controlling properties. Laboratory experiments in concept formation follow this pattern by setting up and testing for the presence of abstract tacts in an artificial verbal community. The same procedures could be used in an empirical survey of abstraction generated by verbal environments outside the laboratory. (Skinner, 1957, pp. 112-113)

Skinner addresses several kinds of methodological issues in this passage. First, Skinner makes a distinction between first-person and third-person analytic statements (e.g., Day, 1977/1992, 1983/1992); an issue which, particularly in the case of "subjective" or "mentalistic" terms, engages the role of private events in the control of verbal behavior (e.g., Moore, 1995; to be discussed below). Second, it would appear that Skinner is addressing a method which is not an example of standard experimental practice. Note that Skinner uses the term "empirical" in place of the standard and usually-preferred "experimental," and that he contrasts the example of the laboratory experiments on concept formation with the notion of an "empirical survey. . .outside the laboratory." Third, while Skinner says that "the basic formula is simple" in terms of the evocation of verbal behavior under stimulus control, he also acknowledges that "there are many technical problems to be solved" in the carrying out of a satisfactory program.

One way to begin the development of a methodological strategy for the functional analysis of psychological terms is to start with the scientific practice of interpretation. Skinner's extensive use of behavior analytic interpretation (e.g., Skinner, 1953, 1957, 1969, 1974) was, of course, derived from abstractions based upon basic labora-

tory research, and in Skinner's later writings the practice of interpretation came to be mentioned more explicitly as a central scientific practice along with prediction and control (e.g., Day, 1976b/1992; Skinner, 1974).

If one is to pursue behavior analytic interpretation as a first step in the functional analysis of mentalistic terms, the principle methodolgical question becomes one of formulating the remaining steps in the analysis. What is to be done with interpretations, and how are they to be evaluated? The question is one of "confirmation"; a topic which has itself been the subject of Skinner's interpretative practices, as in the following passage from the chapter on "Logical and Scientific Verbal Behavior" from *Verbal Behavior* (1957):

> When new verbal behavior has been constructed, it must often be "confirmed." The process is not limited to constructed sentences. We confirm any verbal response when we generate additional variables to increase its probability. . . . Frequently we confirm a response by finding variables which control a similar form of response in some other type of operant. (Skinner, 1957, p. 425)

In the case of a functional analysis of ordinary-language "psychological" terms, it would be possible to confirm an interpretation formulated in the technical vocabulary of behavior analytic science by (a) *synthesizing* and *constructing* the contingencies set forth in the interpretation in a laboratory context, in order to (b) determine whether the verbal behavior of interest (i.e., the "psychological terms") may be *evoked* in the behavior of speakers of the natural language when the speakers are *observing the environment-behavior interactions* thus constructed. In other words, if a functional analytic interpretation of a mentalistic term may be regarded as a technical description of the sufficient conditions under which the term is "used," then it should be possible to construct the conditions and then test whether the term is evoked in the verbal behavior of an observer.

One way to characterize such a methodological strategy is to view it as an analysis of the relations between three classes of interactions (the term "interactions" will be

used here rather than, say, "stimuli," since a given member of a class may function as a stimulus or as a response, depending upon the verbal context, and also to emphasize that the members of a given class are functional only in the context of the interactions and contingencies of a given verbal community or subcommunity; see also discussions by Catania, 1992; Sidman, 1994; Skinner, 1957). One class consists of the "psychological terms"; for example, the terms "purpose" or "purposive act" may be viewed as members of such a class. Such terms, however, are related intraverbally to other classes of verbal behavior, most notably those behaviors which could be described as the intraverbal "meaning" of the terms, as determined by the practices of the verbal community (e.g., Skinner, 1957). One finds equivalences between such classes of verbal behavior, for example, in dictionaries, where (a) a term and (b) its intraverbal "definition" may be said to "mean the same thing"; or that either may be substituted under a given set of appropriate circumstances (and where different sets of circumstances are implied by the different "definitions" listed). Alternatively, one might ask a member of the verbal community to "define" the term "purpose" or "purposive act," or to "say what the term means." The speaker might respond with "It is something one intends to do," "It is an act of volition. . .," "An exercise of the will. . .," and so on.

Essentially, the methodological move proposed in such a "synthesis/evocation" strategy is to assess the functional properties of a third class of interactions; i.e., those interactions which characteristically give rise to the term (or alternatively, the terms characteristic of an equivalent "definition") when the term occurs as a tact (Skinner, 1957). This third class of interactions would involve the environmental conditions (e.g., environment-behavior interactions of an organism under observation) which enter into the characteristic stimulus control of the occurrence of the term as a property of verbal behavior. In the case of "purpose" or "purposive act," for example, it would not be surprising to find such terms to occur under conditions in which operant behavior is observed to be conspicuously and continuously under the control of reinforcement variables over time (e.g., Skinner, 1974; cf. Hayes & Wilson, 1993).

Of course, verbal behavior occurs in continuous interaction with multiple sources of control, and given that the relevant contingencies (derived from the interpretation) could be constructed under conditions suitable for controlled observation, instructions could be designed to function as a thematic probe (e.g., Skinner, 1953, 1957). That is, the instructions or questions would serve as supplementary stimulation for the available verbal behavior to be brought "to strength." Such instructions or questions to the observers (delivered prior to and/or during the observations) may be designed to probe different functional verbal classes "within" the observer's verbal repertoire (e.g., Leigland, 1989).

Further, different or specialized groups within a given verbal community might be employed as observers where specialized or technical terms are of interest; for example, if one were to assess an interpretation of conditions relevant to the problem of "intentionality," conditions might be arranged for observation by philosophers with an appropriate technical history and verbal reperpoire. In addition, while there has been an implicit assumption that the observers in these examples would not have a technical verbal repertoire in the field of behavior analysis, there may be a variety of questions to be addressed through the use of conditional control of technical and non-technical terms (through appropriate thematic probes) for a comparison of the verbal behavior of, say, cognitivist and behavior analytic psychologists. Such comparative analyses might be useful in the study of scientific verbal behavior (e.g., Skinner, 1957, Ch. 18).

A variation on this methodological strategy may also be considered. In Skinner's (1957) discussion of "confirmation," it was

proposed that, "It is useful to maintain the distinction between the confirmation of a tact and of an intraverbal" (Skinner, 1957, p. 426). For example,

> The theory of evolution cannot be confirmed by a set of tacts to the actual events taking place in the remote past, but a single set of verbal responses which appear to be tacts to such events is made more plausible – is strengthened – by several types of construction based upon verbal responses in geology, paleontology, genetics, and so on. Only a current event of the same nature (for example, the appearance or production of a new species under the proper circumstances) would generate a tact of the same form and convert the theory into a fact in that sense. (Skinner, 1957, pp. 426-427)

To this point, the general methodological proposal for a functional analytic research strategy has been what Skinner would have termed, the "confirmation of a tact." That is, the conditions of evocation would involve the setting up of some behavioral interactions for observation in real time. For example, the setting might involve a subject observing a pigeon's behavior in an operant chamber where contingencies are being manipulated, and where the observers have been asked to "explain" the observed pigeon's behavior of pecking the key (e.g., Leigland, 1989). One might thus be able to control the classes of terms to be found in the "explanatory" verbal behavior through the manipulation of the contingencies controlling the behavior of the pigeon under observation. Another possibility might involve the observation of a videotape of human behavior in interaction with contingencies, and so on. In these examples, "mentalistic terms" thus evoked would constitute a variety of the tact relation in the sense that the occurrence of the terms would be under the stimulus control of nonverbal stimuli (Skinner, 1957).

The possible methodological alternative might be the "confirmation of an intraverbal" by arranging for the possible evocation of the "mentalistic terms" through the presentation of a *verbal description of the behavioral contingencies and interactions involved in the interpretation*. In other words, through the use of the functional definitions of the technical terms involved in the behavior analytic interpretation of the "mentalistic term," an example of a behav-

ioral episode might be constructed as a narrative (that is, without the technical terms themselves), followed by (or at least in the context of) the appropriate thematic probes (as instructions, questions, etc.). If the "mentalistic terms" of interest are thus evoked in the behavior of the observer (or in this instance, the reader), the "confirmation" of the interpretation would have been achieved through intraverbal control (following Skinner's example in the quotation above; the term "intraverbal" is characteristically reserved for verbal-to-verbal control of a particular type, but the complexity of the interaction and the lack of formal point-to-point correspondence between stimulus and response may suffice for the present purposes for the extension of the term; cf. Catania, 1992; Skinner, 1957).

An advantage of the latter preparation would be that the procedure could be carried out more conveniently and extensively than the procedure involving the observation of an actual behavioral episode, for example. As a disadvantage, however, it is almost certainly the case that the descriptions necessary for such a procedure would be lengthy and cumbersome. It may be worth noting as well that such a procedure would likely be viewed with great skepticism by mentalistically-inclined philosophers, since it would engage the very criticism raised against Skinner's work that was discussed above; that is, it might be taken as evidence that behavior analytic technical terminology is indeed understandable only to the extent that makes contact with mentalistic terms and concepts, which are viewed as primary (e.g., Dennett, 1978; Flanagan, 1991; in other words, such verbal "translations," since they are symmetrical, may be viewed from the perspective of either direction). The answer to such a criticism would be to demonstrate that unlike the mentalistic terms and intentional idioms of ordinary language, the technical vocabulary of behavior analysis can indeed be "cashed out" as a means of communication allowing for effective action in the arena of

behavioral influence (see also Baum & Heath, 1992).

Complicating Factors

To be sure, the functional analysis of psychological terms is a research program which faces a variety of complicating factors, Skinner's (1945) confidence notwithstanding. Three such factors will be noted briefly. First, there is the role of private events in the control of verbal behavior (e.g., Skinner, 1953, 1957). To the extent that the terms of interest could be characterized as "mentalistic" or "subjective," such events could be expected to be a factor in a functional analysis. Although the role of such events would provide a methodological challenge to a functional analysis of such terms, the problem should not be insurmountable since the relations between private events and verbal behavior is presumed to be established and maintained through contingencies as well (e.g., Day, 1976a/1992, 1983/1992; Moore, 1980, 1990, 1995; Place, 1993; Skinner, 1945).

A second complicating factor involves the complexity introduced by equivalence phenomena. It is clear that equivalence phenomena are important to verbal behavior (e.g., Hayes & Hayes, 1992; Hayes & Wilson, 1993; Sidman, 1994), although much work remains to be done in clarifying the relation between the contingencies programmed in equivalence research and the contingencies involved in verbal behavior as it is observed in real-time contexts (e.g., Leigland, 1991a, 1991b; Rosenfarb, 1992). One theme that has emerged from the recent expansion of equivalence research is that the emergent relations observed between classes of arbitrary stimuli might greatly extend a person's functional (verbal) repertoire without the necessity of direct training histories (e.g., Hayes & Wilson, 1993). Whether and in what ways such complexities would affect a functional analysis of ordinary-language terms, however, is unclear, and is perhaps best regarded as an empirical question.

A third factor which may complicate a functional analysis involves the specifica-

tion of the controlling variables. The problem has been described by Catania (1992) in his discussion of abstraction. After describing Hull's (1920) classic experiment on the formation of "concepts," Catania makes the following observation regarding ordinary-language terms:

> Tacting in this experiment [Hull, 1920] differed from tacting in natural languages in that the basis for many of our tacts cannot be so explicitly defined. For example, we cannot say exactly what properties make an object a chair. A chair may have four legs or stand on a single pedestal, it may have a flat or a contoured seat or back, and it may be constructed from many different materials. We cannot even appeal to its function, because we call some objects chairs although they cannot be sat upon (e.g., a toy chair in a set of dollhouse furniture). (Catania, 1992, p. 238)

In this passage we are able to make contact with the passage from Skinner (1957) quoted above regarding the assessment of abstract tacts. Hull's (1920) experiment is an excellent example of the formation of "abstract tacts in an artificial verbal community," as noted in the above quotation by Skinner (1957), and Catania's example of the chair is relevant to Skinner's subsequent recommendation of "an empirical survey of abstraction generated by verbal environments outside the laboratory" (Skinner, 1957, pp. 112-113). How are we to specify the conditions which may be observed to give rise to the term "chair"? First, it would appear that any purely "physical" specification would not be possible, as indicated by Catania (1992; see also the discussion above regarding physicalistic reductionism; e.g., Leigland, 1993). In what language are we to specify the controlling conditions if not in the language of physics? Further, might not the number and variety of such conditions (in whatever language they are specified) make such an analysis impractical, if not impossible? Further still, if we are to meet such complexities with such "simple" examples of abstract tacts, what may we expect from terms such as "purpose," "mental," or "exist"?

Provisional answers to these questions may be summarized in the following way: functional descriptions (rather than simply or necessarily "physical" specifications) of

stimulus properties (e.g., in the case of "chair") or contingencies (e.g., in the case of "purpose") will serve the purpose of a functional analysis of ordinary-language terms. The issue of whether specification in the language of physics is necessary turns on the issue of precision rather than upon any sort of concern for philosophical "foundation" (e.g., in some experiments on stimulus control involving visual stimuli, the specification of the stimuli is useful or necessary in terms of wavelength; e.g., see Catania, 1992; in other experiments, a more general, natural-language description is adequate; e.g., Ferster & Skinner, 1957). The adequacy of the descriptions would be assessed in terms of their demonstrated capacity to evoke the relevant terms in the verbal behavior of speakers observing the stimulus properties (or the synthesized and constructed contingencies, or verbal descriptions of these) in question (thus providing a "confirmation" of the interpretation/description). Although there may be a variety of such conditions or contingencies entering into the control of such terms, the conditions may nevertheless be classed, organized, and summarized for various purposes.

The present issue might be summarized by returning to the above quote from Catania (1992). In the example of the term "chair," to say that "we cannot even appeal to its function" in the exact specification of controlling variables is to recognize that the term serves more functions than may be seen in the context of its "generic usage"; other functions of the term (as when it occurs in the context of dollhouse furniture) demonstrate other types of extension of the term as an abstract tact (e.g., Skinner, 1957). In either case, the controlling variables could be clarified through an appropriate functional analysis.

More generally, we may also agree with Catania (1992) that we "cannot say exactly" nor can we define "explicitly" the properties that enter into the control of a particular abstract tact without a thorough, empirically-based functional analysis of the abstract tact as a property of verbal behavior. For practical purposes, it may not be possible to "exactly" or exhaustively state all such conditions for a given abstract tact, since (for example) any such tact would presumably be evolving in function through the processes of extension (Skinner, 1957); a phenomenon perhaps related to those studied in research on equivalence classes (cf. Hayes, 1994). Nevertheless, a functional analysis might make substantial progress in identifying and clarifying such controlling conditions, and in doing so (among other possible byproducts), provide a challenge to traditional notions of language and "meaning" (another term which would constitute a prime candidate for a functional analysis; e.g., Skinner, 1945, 1957; cf. Day, 1969a/1992; Murphy, 1990; Rorty, 1991; Wittgenstein, 1953).

Exploratory Studies

Several empirical studies have explored analyses similar to those described here. For example, Leigland (1989) analyzed some of the behavioral conditions which give rise to a broad class of "mentalistic" terms in human observers. Undergraduate observers had been instructed to "explain" the behavior a pigeon pecking a response key in an experimental chamber, using "whatever terms or phrases...[that] feel 'comfortable'..." (Leigland, 1989, p. 6). Subjects were to write their explanatory statements (of whatever length) on a clipboard at any time during the session. Each statement was to be numbered, and whenever they were writing, they were to hold a handswitch closed with their non-writing hand. In two experiments, subjects observed the pigeon's keypeck response as controlled in real-time by specific contingencies of reinforcement (a fixed-interval schedule, or relatively precise discriminative stimulus control). The subjects' handswitch controlled an event pen on a cumulative recorder which was used for recording both the explanatory "events" of the subject as well as the keypecking behavior of the pigeon throughout the session. After the session, any terms in the explanatory statements designated as "mentalistic" could be matched up with

the corresponding explanatory statement "event" on the cumulative record, and placed directly upon the cumulative response curve above the moment that the statement "event" had been initiated by the observer.

The goal of this procedure was to search for orderly relations between the environment-behavior interactions under observation (in this case, as shown on the cumulative record), and the occurrence of "mentalistic" terms in the written verbal behavior of the observers. Several kinds of discriminations could be made on the basis of the results from the two experiments (see also Dougher, 1989).

In addition, a difference between the two experiments gave rise to an interesting possibility. In the first experiment, the ongoing keypecking behavior of the pigeon was relatively undifferentiated over time; in the second experiment, the keypeck response was under relatively precise stimulus control. The first experiment yielded frequent "mentalistic" terms in the explanatory statements, while relatively few such terms occurred in the second experiment. In the latter case, statements of a more "descriptive" character were observed in the explanatory statements (or statements which might be considered as closer to "pure tacts"; Skinner, 1957). While direct comparisons of the two experiments cannot be made, it was suggested by Leigland (1989) that an area for further investigation might involve the conspicuousness of the observed contingencies. That is, mentalistic explanations may be more likely to occur in behavioral situations where sources of control, or the interaction of the behavior with controlling contingencies, are difficult to observe or to discriminate. Behavioral contexts in which control is conspicuous or more clearly discriminable might be less likely to occasion mentalistic explanations. A recent doctoral dissertation by Mueller (1995) has provided some support for this interpretation (the study by Leigland, 1989, bears similarities to a procedure developed independently in a doctoral dissertation by Lahren, 1978; see also a doctoral disserta-

tion by Mascolo, 1985). Such studies constitute the first approximations to the sort of research program described by Skinner (1945).

CONCLUSIONS

Many of the present proposals may be summarized through the use of an illustration involving the analysis of equivalence phenomena (e.g., Sidman, 1994). It was noted above that the synthesis/evocation strategy of "confirming" behavior analytic interpretations of psychological terms involved the relations between three classes of interactions. One set of relations has been previously established by the practices of the verbal community; namely, the symmetrical relations between the "psychological terms" on the one hand, and the intraverbal "meanings" on the other (see also Sidman, 1994). In one sense, the goal of the synthesis/evocation strategy may be described as the establishment, through empirical means, of a new set of symmetrical relations; namely, the relations between the "psychological terms" on the one hand, and the environment-behavior contingencies which may be observed to evoke those terms on the other.

More precisely, while the latter relations should also be regarded as having been previously established by the practices of the verbal community, it is the purpose of the functional analysis to make those relations *conspicuous* and available for direct observation and analysis. One half of this symmetrical relation may be identified as the practice of interpretation and synthesis (e.g., as when the terminology of "purpose" is interpreted as under the control of certain contingencies of reinforcement, etc.; e.g., Skinner, 1974; cf. Hayes & Wilson, 1993). The other half of the symmetrical relation may be identified by the evocation of the term or terms by the synthesized contingencies (as when we "confirm" the interpretation by evoking the "purposive" verbal behavior by the observation of behavior in contact with the relevant contingencies, etc.).

We may follow the example of equivalence phenomena to take an additional

step. It is the hallmark of equivalence phenomena that given certain training histories setting up certain controlling relations between different sets of arbitrary stimuli, additional controlling relations between the stimulus classes may be observed to have "emerged" in the absence of direct training (e.g., Hayes, 1994; Sidman, 1994). To cast the current proposals into the format of a standard equivalence experiment, having observed symmetical relations between the terms and the intraverbal "meanings" constructed by the verbal community, and having *demonstrated* the relations between the terms and the controlling contingencies through an empirically-based functional analysis, it may be reasonable to ask whether the relations between the intraverbal "meanings" of the terms and the controlling contingencies might become apparent through the emergence of a derived relation. This step is, of course, speculative, but it may nevertheless lend itself to empirical research, as some studies have begun to extend the domain of equivalence phenomena to classes of terms found in natural language (e.g., Kohlenberg, Hayes, & Hayes, 1991). If such derived relations were found to occur in such verbal contexts, they could greatly facilitate the recognition of the role of contingencies in verbal behavior.

The purpose of this paper has been to support the contentions of Skinner (1945) concerning the feasibility of the functional analysis of psychological terms as a program of empirical research. While it is clear that, as Skinner (1957) noted in the quotation cited above, "there are many technical problems to be solved before this can be done on a satisfactory scale" (pp. 112-113), we have examined several methodological issues and possible strategies.

It has also been the purpose of this paper, however, to take issue with Skinner's (1945) assessment of the research program as being "of historical interest only" (p. 292). Such a research program would bring behavior analytic science into direct contact with verbal practices of central importance to the verbal community (e.g., those concerning "mind" and "mental life"; e.g., Gardner, 1985; cf. Skinner, 1964, 1974,

1989), and would extend the functional analysis of verbal behavior more generally; for example, by extending the analysis through methodological development, and through the demonstration of the conspicuousness and ubiquitousness of behavioral contingencies. Although it is clear that a great deal of basic, applied, and clinical research is needed in the analysis of verbal behavior, it is proposed that to those programs we may also add the functional analysis of psychological terms as a means of joining the analysis of verbal behavior to the practices of the verbal community in which it functions.

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